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Letters to the Editor

New findings concerning early bone grafting procedures in patients with cleft lip and palate

E. Keese, R. Schmelzle

Journal of Cranio-Maxillofacial Surgery 23 (1995) 296–301.

Sir

I have read with interest the paper by Keese and Schmelzle. The authors have evaluated 177 primary bone grafted clefts of the alveolus in 148 patients from the Hamburg Cleft Center data bank. Interesting radiographic long-term results were found and published in this article. Two to three questions come to my mind:

- 1.1. How many alveolar clefts had received primary bone grafting in Hamburg in the time span between 1959 and 1969? I know from other publications that many more clefts of lip, alveolus (and palate) had been operated upon in Hamburg than just 148 patients.
- 1.2. How many of these patients had received an additional bone graft? Apparently most of them, otherwise they would have been evaluated as well in this paper. Thus not only 70% of these patients (as stated by Keese and Schmelzle) should have had, but a much higher percentage of the grand total should have had, or has already had, an additional bone grafting procedure.
2. The authors of this paper discuss in length the possible disadvantages of early primary bone grafting with regard to sagittal facial growth. They also cite H. Scheuer et al. who have apparently evaluated the same patients, and who have noted deficient sagittal growth as a result of early primary osteoplasty. Here they are citing Scheuer et al. wrongly, when stating that no difference was found in sagittal growth between the cases with primary (early) bone grafting when compared with those with secondary (late) bone grafting!

The interested cleft surgeon regrets the lack of this important data in the valuable publication by Keese and Schmelzle. After all this was the crucial point that made most cleft centers in the world cease performing primary osteoplasty after Rehrmann, Koberg and Koch had published their negative long-term results (Rehrmann et al. 1973).

I hope my most honorable colleagues will excuse this letter of polite dissent and I am looking forward in anticipation to reading their reply.

Reference

Rehrmann, A., W. Koberg, H. Koch: Die Auswirkungen der Osteoplastik auf das Wachstum des Oberkiefers. Fortschr Kiefer-u. Gesichtschir XVI/XVII: 102–108 (1973)

Prof. Dr. Dr. K. Gundlach

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Reply

We thank Prof. Gundlach for his questions and hope to clear up potential misinterpretations with this reply.

Primary single stage bone grafting in cleft surgery was introduced at the Nordwestdeutsche Kieferklinik in Hamburg in 1959. In the early 1970s, as mentioned by Prof. Gundlach, the use of this Hamburg surgical method was restricted – and later discontinued – on the basis of the results obtained by the Maxillofacial Surgery Dept. in Düsseldorf (Koberg 1971). The last primary bone grafting procedure with rib grafts was carried out in 1982.

A change in indication for timing of alveolar cleft grafting after 1970 led to limitation of our investigation to the time span 1959–1969. We invited all 635 bone grafting procedure patients from this period to undergo a clinical investigation (question 1.1). One reason why only 148 patients responded could be the lengthy postoperative period of up to 35 years, during which most patients will have changed their place of residence.

We applied no selection criteria to these invitations (question 1.2). Patients who had undergone an additional bone grafting procedure were also invited, investigated and classified as Bergland Index IV (failure) as described in our article under Method.

Independent of our investigations, we knew that 112 of the 635 patients (18%) had received at least one bone graft. We assumed, however, that the proportion of patients with a reduced interdental septum was larger and indicated this in our article (Bi III = 28%; Bi IV = 42%).

The question that must be asked is why only 18% received additional bone grafts although the indication had been diagnosed in 70%.

In retrospective investigations, it is always problematical to obtain a representative selection of all patients. With regard to the 18% figure, this was possibly also the case in our article. In contrast to Prof. Gundlach, the more likely conclusion is that our assessment of primary bone grafting was surely too negative.

As for question 2, we can only reply that we cited Scheuer et al. (1993) accurately. At 16 years of age they determined an SNA of 75.1° in the late osteoplastic group versus 74.9° in the primary bone grafting group. The difference of 0.2° was not significant. Both groups showed a tendency towards a retrognathic profile. Scheuer et al. (1993) thus not only found no difference between primary and late bone grafting procedures, but also that treatment of these

patients should not be based on standard non-cleft patient data.

Independently of this, we have checked the Scheuer et al. (1993) study and are in a position to confirm their results.

The article we cited is five pages before the publication of Gundlach et al. (1993). Our discrepancy can surely be cleared up.

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- Koberg, W.: System der Rehabilitation von Patienten mit Lippen-Kiefer-Gaumenspalten. Westdeutscher Verlag, Opladen, 1971
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The sandwich zygomatic osteotomy

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Sir

The article deals with a series of 20 patients with developmental facial deformities, which were treated by bilateral sandwich zygomatic osteotomy. The authors described in detail the technique used in combination with well illustrated pictures. In the literature list, I miss the article of Eduardo J. Gimenez (Osteotomia del hueso malar) who published the technique in 1973, where similar combined oblique and vertical osteotomy lines are described and illustrated. The current authors of this article used an interpositional porous hydroxyapatite block to maintain the laterally displaced greenstick fracture that occurs at the temporo-zygomatic suture. In the article described by Eduardo J. Gimenez, the resultant greenstick fracture at the temporo-zygomatic suture is stabilized by a F. Urban splint. We indeed agree, replacing the F. Urban splint by an interpositional graft. The experience teaches that the use of autologous bicortical bone harvested from the chin gives satisfactory results, even if a genioplasty is not necessarily indicated.

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Reply

In his letter, Dr Walji has raised two issues: firstly, the origin of the technique and secondly, the stabilization of the valgus position.

Our article concentrated on a technique for bilateral malar augmentation in cases of developmental hypoplasia. Of course, numerous osteotomies are described to reposition a traumatically displaced zygoma. Dr Walji points at such an osteotomy type, described in a 1973 issue of the *Revista Assoc. Odont. Argent.* by Dr E. Gimenez. It concerns a report on four cases of unilateral posttraumatic malar impaction, partially corrected by performing a vertical, infraorbital and oblique chisel osteotomy in the posterior sinus wall. The infraorbital nerve is exposed, the masseter muscle insertions are stripped off, the zygomatic arch and the malar body is dissected subperiosteally along its entire facial surface. Stabilization is performed with an acrylic block in the infraorbital osteotomy gap. I would not advise the readers to correct post-traumatic malar deformities in such a way. Aseptic necrosis of the zygoma, sinusitis induced by the foreign body, and especially persistence of the enophthalmos, canthal dystopia and infraorbital rim recession, are potential complications. Maybe for these reasons, long-term results are not discussed in Dr Eduardo J. Gimenez's article.

Respecting the editor's desire that '... authors must refrain from citing too many references in the article ...' (Editorial, *J. Cranio- Max.- Fac. Surg.* 22 (1994) 1) I try to refer to articles that are directly related to the topic discussed. The literature search is done accordingly, with the languages English, German, French, and Dutch as four more restricting parameters.

I agree that autologous bone should be the first choice material in the correction of facial skeletal deformities, whatever the cause may be, congenital, developmental or posttraumatic. Therefore, I sincerely hope that Dr Walji will be able to report less resorption rates with bone grafts when used for the malar body displacement than Drs. Vargervik, Farias and Ousterhout had for the stabilization of the zygomatic arch displacement (*J. Cranio- Max.- Fac. Surg.* 15 (1987) 208–212).

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